

REMINISCENCES

I was an undergraduate student at Kanazawa University, which had been recently established as part of the post-war educational reforms. Many of the professors had moved from the old imperial universities and still followed the old curricula. A few among them formed a research group for theoretical particle physics. Since there were no students senior to us nor graduate students, a few of us were made welcome in their reading club. There, for the first time I was exposed to research in theoretical particle physics.

Since Kanazawa university was an undergraduate college at that time, I went to Nagoya university for my graduate studies. Before going to Nagoya I was already engaged in some calculations that I had been asked to carry out by Oneda, my mentor at Kanazawa. These were $\Lambda - \beta$ decay calculations, and the results were published in a joint paper by Iwata-Okonogi-Ogawa-Sakita-Oneda, a paper that dealt with the universality of weak interactions and in a sense, adumbrated the universal V-A interaction. The architects of the paper were Shuzo Ogawa and Sadao Oneda, from whom I learned the phenomenology of strange particles (then called V-particles) and the weak interactions. This was about the time that the strangeness theory was put forward by Nakano-Nishijima and by Gell-Mann.

In Nagoya at that time, each graduate student belonged to a research group. I belonged to Sakata's group. I stayed there for two years, and received a Master's degree in 1956. As I look back now, this was one of the most fruitful periods for Sakata's group. The Sakata model was proposed in my second year, although I was not a part of this activity. By then I had become more and more interested in the collective model of nuclei and the work in Nagoya by Marumori and others. My master's thesis was on collective motions.

In 1956 I went to the University of Rochester. When Robert E. Marshak, then the chairman of the physics department at Rochester, visited Japan to attend an international conference in 1953, he expressed his interest in having a number of Japanese graduate students join his group at Rochester. To this end, he requested Yukawa and Tomonaga to select some students. Fourteen students were selected between 1953 and 1959, I being one of them. I received a research assistantship and a Fulbright travel grant.

In Rochester, I had to take regular courses during the first year. Among these courses, I had to take an experimental course; "Modern Physics Laboratory." Before coming to Rochester I had been entertaining the possibility of going over to experimental physics, as Koshiba and Yamanouchi had done. After an unsuccessful X-ray experiment in that course, however, I gave up that dream.

In the spring of 1957 there was a Rochester Conference which graduate students were allowed to attend. A highlight of the conference was Lee-Yang's work on parity non-conservation in weak interactions. It was very exciting to see all these noted people whom I knew only by name. There were many activities in Rochester going on then too, such as the V-A theory of Marshak and Sudarshan and the Marshak-Signel nuclear potential, to name a few. However, I, being timid and merely a new graduate student to boot, could not participate in them. It was very frustrating.

While I was at Rochester, I more or less followed the topic that was current – dispersion relations and symmetries (global symmetry, pre-SU(3)). Dispersion relation was the

subject that I had never studied in Japan. While I was studying its techniques, I tried to apply it to various problems. The first attempt was on the $K_{\mu 2}$ decay where I mimicked the Goldberger-Treiman calculation of the π meson decay. This calculation led to the conclusion that the strangeness changing current is much weaker than the strangeness conserving current, an indication of Cabibbo mixing. Around this time, Marshak's group was working on nuclear forces, computation of nucleon-nucleon phase shifts, the photo-disintegration of deuteron, etc. J. J. de Swart, a student of Marshak, was working on the photo-disintegration of the deuteron and I was attracted to the subject. I discussed with Susumu Okubo, a research associate then, regarding the possibility of using dispersion relations in this problem – and eventually it became a part of my thesis at Rochester. My adviser was Charles J. Goebel, then a young assistant professor, with whom I finished my Ph.D. in 1959. He gave me complete freedom in physics research and provided appropriate advice whenever necessary. During my three years at Rochester, I received much encouragement from my fellow Japanese graduate students, and I also learned a great deal of physics from them, especially particle theory from Susumu Okubo and particle experiments from Taiji Yamanouchi.

I took a postdoctoral job at the University of Wisconsin and moved to Madison in the summer of 1959. While I was in Rochester I did not work on weak interaction phenomenology, but I maintained an interest in that subject. In Madison I resumed research on strange particle decays and collaborated with Oneda and Pati of Maryland through correspondences. This work is a precursor to the Penguin mechanism of the $|\Delta I| = \frac{1}{2}$ enhancement in non-leptonic weak interaction.

My boss at Wisconsin was Robert G. Sachs, who was keenly interested in high energy experiments and had helped to build up a strong experimental group. With his encouragement, I developed a friendship with the experimental group, in particular with M. L. Good and W. D. Walker. Through the next several years I closely observed the excitement they felt in the discovery of hadronic resonances. At about this time the Fry-Camerini group erroneously claimed the discovery of $\Delta S = -\Delta Q$ events in neutral K meson decays. Sachs had strongly supported the claim and had created such an atmosphere that for the next several years the theorists in his group could not discuss models of weak interactions without $\Delta S = -\Delta Q$. Most of the models, based on the constituents of hadrons such as the Sakata model or the quark model, became extremely ugly. I spent fair amount of time and energy on the model building of weak interactions only to be considerably frustrated. This was the last time I was engaged in weak interaction physics.

In Madison the Summer Institute was regularly organized by Sachs. Attending the 1961 Summer Institute, I keenly felt my lack of knowledge in group theory. I only knew angular momentum at the time. In the summer of 1962 we did not have the Summer Institute, but Jan Tarski, then a postdoc at the Institute for Advanced Study (IAS) in Princeton, visited Madison and he and I shared an office. Since I knew that he had given a seminar on group theory in the previous year's Summer Institute, I thought it was a good opportunity to learn group theory from him. Although I wanted to know the representations of SU(3), I asked him about SU(4) instead, because I was shy about

revealing my intention and moreover thought that if I understood "4" I would understand "3". He then started explaining it at great speed by drawing a lot of dots on the blackboard. But, when he found me completely foxed, he looked at me pityingly and said that the representations of $SU(4)$ had all been worked out by Wigner. This is Wigner's famous paper on the supermultiplets of nuclei. Immediately I went to the library. Although I did not quite understand the group theory part of the paper, I understood very well the intent of the paper, in particular the introduction, where Wigner explains why and how group theory is applied to this problem.

My teaching career began in 1963. That year I taught a course called "Special Topics in Theoretical Physics"; the weak interactions in the fall semester and the strong interactions in the spring semester. This was the year of $SU(3)$; the Cabibbo theory of weak interactions and the quark model. I reviewed in detail the status of elementary particle research at that time including some of my own contributions. In the last lecture of the course I discussed the non-relativistic $SU(6)$ theory, a supermultiplet theory of hadrons based on the non-relativistic quark model, which I had developed during the second term (Spring 1964). I was well aware of the limitation of the theory, especially the difficulty associated with the relativistic generalization. I worked on this problem all the time during the summer of 1964. I presented my version of $SU(6)$ in the last seminar of that year's Summer Institute. Louis Michel and Eugene Wigner were there in the audience. After the seminar Wigner came back to the seminar room and informed me of the news he had received from Gürsey and Radicati on their independent work. Next day Wigner invited me to his office, and grilled me with several questions before convincing himself that I had used the same $SU(6)$ group as had Gürsey and Radicati, and then he opened a briefcase to show me their paper. By then I knew the content of the paper because I had called Feza Gürsey the previous day and had learned it from him. There was an important difference, however, between the two. The difference was that I had chosen the anti-symmetric representation "20" of $SU(6)$ for baryons based on the naive quark model, while they correctly chose the symmetric representation "56". During the summer I had been so preoccupied with the relativistic problem of $SU(6)$ that I had neglected to examine the $SU(3)$ contents of some of the possible representations of $SU(6)$. When I expressed my concern about the relativization, Wigner politely refused the discussion and suggested that I discuss the matter with Louis Michel.

A few days after the seminar, I moved to Argonne National Laboratory, where Sachs became the associate director of the laboratory in charge of high energy physics research and I joined its theory group. The first thing I did in Argonne was a quick calculation of the nucleon's magnetic moment using the representation "56". When I found the ratio $-3/2$ for the magnetic moment of the proton and the neutron, I became fully convinced of the correctness of the "56", and thought that we might have to abandon the naive quark model. As is well-known, the resolution of this dilemma later led to the introduction of the color degree of freedom, and eventually to QCD.

In Argonne, before my family and I found a house in a nearby town, we stayed for one month at the visitors' housing facility. Harry Lipkin, who showed a strong interest in $SU(6)$, was also there with his family. A few days later Michel and his family moved from Madison and stayed in the facility for a month or so before going back to France.

I started to discuss with Michel about the feasibility of relativistic $SU(6)$. But we had communication problems, because his group theory was much too sophisticated for me. Nevertheless, we managed to finish a work, which dealt with a discussion of, together with some negative results in the relativistic extension of the $SU(6)$. A joint paper, in actuality written by Michel entirely in his own style, was drafted. To tell the truth, he had agreed to write a more elementary paper with me by using Lie algebra. But, we have never finished it mainly because he was satisfied with his version and I became extremely busy working on the next project with Kameshwar C. Wali.

This was the time when Argonne played the role of a center of high energy physics activities in the Midwest. Many physicists from the nearby universities gathered at the weekly seminars. Yoichiro Nambu and Jun J. Sakurai showed up quite frequently. I had known Nambu for some time since he was a frequent visitor to the Summer Institute at Madison. In discussions with him I myself became convinced that an attempt at a phenomenological but relativistic formulation of $SU(6)$ would be worth a try, inspite of the negative results we had had regarding $SU(6)$ being an essential theory. K. C. Wali and I started working on this intensively in the late fall of 1964, and we finished the work just before the 1965 Coral Gables Conference in January, to which I had been invited. The first speaker of the conference was Abdus Salam, who spoke on the relativistic $\tilde{U}(12)$ theory of Salam-Delbourgo-Strathdee. I was shocked by the talk since their work was identical to ours, even to the notations. During the talk W. D. McGlinn, who knew our work and was sitting in front of me, turned around several times and kept gesturing to me to speak up. After the talk I spoke up and then handed a hand-written copy of our paper to Salam. At a party that evening Salam returned the paper back to me saying that both works were the same. And then he invited me to visit the International Center for Theoretical Physics (ICTP) where he was the director. In the following week at the APS meeting in New York, Pais presented a work with Beg on their version of the relativistic $SU(6)$, which subsequently was reported in the New York Times. A few weeks later, I saw Salam's picture in an English newspaper that had been posted on a bulletin board in Argonne. I was not too happy about the fact that our contribution was not suitably acknowledged. Moreover, the publicity accorded to a work that I regarded as merely phenomenological, made me quite uneasy.

In the same Coral Gables conference, Roger Dashen gave a talk on the bootstrap program, in which he was describing the various vertices in terms of the matrix elements of a few matrices. After I came home I realized that Goebel's strong coupling theory could be formulated algebraically by using matrices, and the next moment I had obtained a Lie algebra of the symmetry group which serves as a spectrum generating algebra for isobar states (hadrons). I immediately informed Goebel, my advisor at Rochester, of this development. In the meanwhile, in 1961, he had joined the faculty at Wisconsin. In Argonne at that time there were several group theorists: Morton Hammermesh, William D. McGlinn, and the mathematician Robert Hermann. When Michel was still there, he proposed having a series of tutorial lectures by Hermann. Based on these lectures Hermann later wrote his well-known Benjamin book. When I told Hermann about my findings on the strong coupling group (as we named it), he showed a strong interest in it and suggested that I study a few relevant mathematical subjects: group contraction, induced representation,

and the Peter-Weyl theorem. This was a very valuable suggestion, for Thomas Cook (my first student) and I spent the next one year or so working on these problems. Meanwhile, we published a short paper (Cook-Goebel-Sakita) on the strong coupling group together with a derivation of the representations for a few simple cases by using the method of contractions.

Suddenly the $SU(6)$ became fashionable and I started receiving many invitations. In the summer of 1965 I was invited by Delhi University to give a series of lectures in a summer school at Dalhousie, a hill station in the Himalayas. Using this opportunity, I traveled around the globe: India, Japan and England in that order to participate in summer schools and conferences. This visit to Japan was the first since I had left nine years previously. I was well received there and I really felt the difference that the $SU(6)$ had made.

In the spring of 1967 I stayed at the ICTP in Trieste for five months. Towards the end of the stay K. C. Wali and I traveled to Israel, specifically the Weizmann Institute for ten days at the invitation of H. J. Lipkin. When we arrived in Israel we found that the atmosphere was extremely tense and people busy preparing for a war with the neighboring Arabic countries. Although the touristy places were deserted, we could manage to rent a car to visit many places including Jerusalem, Haifa and Acre. Since most of the young Israeli physicists had already been drafted, the physicists working at the Institute were mainly foreigners, among whom were H. Rubinstein, G. Veneziano and M. Virasoro. They were working together on superconvergence relations, which was a subject that I was also interested in at that time. In the discussions we had during this visit, the dual resonance program must have come up, since I remember that afterward in Trieste I started discussing with others about the possibility of constructing scattering amplitudes by summing only the s-channel resonance poles. We left Israel as scheduled on June 4 and the very next day in Ankara, Turkey we heard of the outbreak of the Six Day War.

In 1968, Keiji Kikkawa and Miguel Virasoro, with whom I had become acquainted in the previous trips to Japan and to Israel, joined our group at Wisconsin as research associates. By then I had returned to the University of Wisconsin to resume teaching, which I missed at Argonne, and I was preparing a course, “Advanced Quantum Mechanics”, which was essentially a one-year graduate course on quantum field theory. In that summer Virasoro showed up in Madison with a hand-written paper by Veneziano and he explained to us in detail the activities of the Weizmann Institute. At once Goebel and I got interested in the work and all of us started thinking about generalizations. In that fall after Virasoro succeeded in obtaining the five point Veneziano formula, our activity became intensified and within a few weeks Goebel and I had obtained the N point Veneziano formula. Then Kikkawa, Virasoro and I started to generalize the formula further to include loops.

At this point we faced a dilemma. Namely, if one considered the Veneziano formula as a narrow resonance approximation of the true amplitude as was commonly assumed at that time, the construction of loop amplitudes based on this approximate amplitude did not make sense. After reviewing the logistics of quantum field theory, we arrived at the conclusion that the construction of the loop amplitudes did make sense if we considered

the Veneziano amplitude as a Born term of an unknown amplitude for which we had an expansion similar to the standard Feynman-Dyson expansion in perturbative field theory. With this philosophy in mind we decided to construct a new dynamical theory of strong interactions. First we defined the local duality transformation as the crossing transformation at any four point sub-diagram of a Feynman diagram, and invented a Feynman-like diagram compatible with duality as a diagram which contained all the Feynman diagrams related to each other by the local duality transformations. Then we simply wrote down a prescription for the scattering formula corresponding to each of these Feynman-like diagrams. In practice, we used diagrams which were dual to the Feynman diagrams. A three point vertex of a Feynman diagram corresponds to a triangle in the dual diagram. An N point Feynman tree diagram corresponds to a specific triangulation of an N polygon in the dual diagram. A local duality transformation in the dual diagram is the transformation of one triangulation of a quadrangle to another triangulation. In terms of the dual diagram, therefore, an N point Feynman-like tree diagram corresponds to an N polygon. By studying these Feynman-like diagrams, it became clear to us that a dual amplitude corresponded in a one-to-one fashion to a two-dimensional surface with boundaries, and equivalently to a Harari-Rosner quark line diagram, which, by the way, we had also invented independently. In the second paper, we discussed the general Feynman-like diagrams by using the classification of two dimensional surfaces, and extended the prescription to non-planar diagrams. This classification is the same as that of open string amplitudes.

Kikkawa left Madison in the summer of 1969 for Tokyo, and Virasoro and I left the following summer bound for Berkeley and France respectively. By then the operator formalism of the dual resonance amplitude had been established by S. Fubini, D. Gordon, and G. Veneziano, and independently by Y. Nambu, who further proposed the string interpretation based on this work. When I heard of the string interpretation from Nambu I felt it as natural as if I had known about it beforehand. I remember that I had experienced the same feeling when I had first heard about the Sakata model from Sakata.

At Wisconsin Virasoro had used the operator formalism to analyze the possibility of the negative metric ghost states consistently decoupling from the physical states. He obtained a set of operators, which could be used consistently as the operators of subsidiary conditions on the physical states. These operators were later found to be the generators of conformal transformations on a complex plane. This is the origin of the Virasoro algebra. In discussing this problem with him, I realized that these operators were compactly expressed in terms of a scalar field in a fictitious 1+1 space(finite)-time, and the Veneziano formula itself could be expressed in terms of this scalar field operator. At about this time we received a hand-written paper by H. B. Nielsen: "A physical model for the n -point Veneziano model." Inspecting a few mathematical formulae in the paper, I came up with a functional integral representation of the Veneziano formula. There remained several important points to be clarified, such as the Möbius invariant property of the functional integrand, the connection with the operator formalism, and the calculation of non-planar amplitudes. At the end I, together with Virasoro and my student C. S. Hsue, established the functional path-integral formulation of dual resonance amplitudes, and with Virasoro, a physical model of the dual resonance model based on the "fishnet" diagram.

I stayed in France for one year before I moved to the City College of New York in

1971. To lessen the financial burden on the Institute, Michel had arranged a joint invitation by his Institute at Bures-sur-Yvette and the Bouchiat-Meyer group at Orsay, a group of physicists that later moved to the École Normale Supérieure in Paris. This arrangement turned out to be a very fortunate one for me, as in Orsay I found several young physicists, who were interested in our work. Moreover it was there that I succeeded in starting a long and fruitful collaboration with Jean-Loup Gervais. During this visit, I wrote three papers with Gervais: on the functional integral, conformal field theory, and the super-conformal-symmetry, all in connection with the dual resonance model.

In Wisconsin I had already started working on the factorization of dual resonance amplitudes using the slicing and sewing technique of functional integrals. I had drafted the preliminary results into a paper and had sent it to the Physical Review before I arrived in France. At Orsay, however, I withdrew the paper, as a result of discussions with Gervais, when I convinced myself that a part of the paper was wrong. There were plenty of technical difficulties, on which Gervais and I had to spend another half a year of hard work. In this work we used formally and fully the conformal transformation properties of functional integrals without seriously questioning their validity. Sometime later we suspected the existence of an anomaly, that would explain the critical dimension of the model. I regret that we did not pursue it further.

When I received a paper on the new dual pion model of Neveu and Schwarz in the spring of 1971, I noticed at once that the most important ingredient in the model was the conformal invariance property. One could discuss about the generalization of dual resonance amplitudes in the very general terms of conformally invariant field theories. So, Gervais and I got busy constructing conformally invariant field theories. In this work, we discussed first a general theory of conformal fields by defining the irreducible fields (now known as primary fields) and the conformally invariant Lagrangian, and then we established the functional-integral representation of Neveu-Schwarz model by introducing a fermionic field in the model in addition to the old bosonic field. After the work was completed I wrote a letter to Virasoro (in Berkeley then) informing him of our work, since I heard that he had presented a similar work at a conference in Israel. In the exchange of letters, I learned the Ramond model from Virasoro and that it also could be described by the same Lagrangian simply by changing the boundary condition on the fermionic field.

Gervais and I thought that in the functional-integral representation the elimination of ghost states could be done by factoring out the negative metric components of the fields by using conformal transformations as was done in the standard gauge field theories. The necessary condition for this is, of course, that the Lagrangian is invariant under the conformal transformations. Once we introduced a new field in the new model which generated new ghost states, we had to find out a new set of gauge transformations under which the Lagrangian was invariant. Neveu-Schwarz-Thorn had just published a paper in which they proposed a set of operators to be used as the subsidiary gauge conditions on the physical states of the dual pion model. We tried to interpret these operators as the Fourier modes of the Noether current associated with the new gauge transformations which involved the new fermionic field, and arrived at the superconformal gauge transformations, under which the Lagrangian we had obtained previously was invariant. I believe that these field transformations are the first instances of supersymmetry transformations in a local

field theory. The day after we had drafted this paper, I left France for New York. In this work, we had to use anti-commuting c-numbers (Grassmann numbers) and functional integration of fermionic variables. These, to us were new concepts and we were initially reluctant to use them. Apparently, others shared this reluctance and this work and the functional-integral work in general, was not appreciated in our circle. However, I received an impression that when I presented the work later in December at the conference on functional integration at the Lebedev Institute in Moscow, it, as well as the use of anti-commuting c-numbers was well appreciated.

When R. E. Marshak became the president of the City College in 1970, I, together with Keiji Kikkawa, accepted a position there. I continued my research on dual resonance theory for a few more years, after I had settled down in the City College. There was a big difference, however, between before and after coming to the City College. Although several faculty members were already there before I came, I was expected to play the role of the leader of the high energy theory group. I felt that it was a great challenge to elevate the group into a quality research group. In a few years, thanks to Marshak's personal connections, we could gather a few talented graduate students into our group. And also we could hire a new faculty member, Michio Kaku and postdocs, such as Yoichi Iwasaki. Moreover, I could invite J.-L. Gervais for short visits on a few occasions. I intentionally spent more time with students, and shared my insights with them.

In the early spring of 1973, I was invited by Ziro Koba to visit the Niels Bohr Institute in Copenhagen for two weeks to deliver a colloquium, and more importantly to discuss the dual resonance string theory with his group, in particular with Holger B. Nielsen and Paul Olesen. By this time at the City College, Gervais and I had already formulated the ghost free Veneziano amplitudes by using the functional-integral representation of the Nambu-Goto string in the light-cone gauge. This work later led to Mandelstam's factorizable functional formulation of light-cone string theory, and eventually to Kaku-Kikkawa's light-cone string field theory. Furthermore in our group at that time, the work of Iwasaki-Kikkawa was near completion. This was an attempt, which I persuaded them to carry out, at a formulation of a light-cone string theory for the Neveu-Schwarz model. I reviewed these activities in Copenhagen. While I was in Copenhagen, David Olive called me up asking me to visit CERN on the way back home. At the CERN seminar, I reviewed the Iwasaki-Kikkawa theory. Later, I was told that this seminar and a conversation after the seminar had led Wess and Zumino to start their seminal work on supersymmetric field theory. I vividly remember the conversation with Zumino at the CERN coffee lounge. When I said, "If you allow me to use anti-commuting c-numbers, Gervais and I have written down a transformation of a fermi field to a bose field in the Nuclear Physics paper", he replied, "It's OK to use anti-commuting c-numbers. Schwinger has frequently used them."

In the June of 1968 there was an international symposium at the ICTP celebrating its new building at Miramare. At the symposium I was introduced to Faddeev and from him I learned the Faddeev-Popov trick. Being fascinated by the method I tried to use it in various problems, and gradually I convinced myself that the method could be useful for a much wider class of problems than simple gauge fixing.

My encounter with the strong coupling theory of Wentzel goes back to my student days at Rochester. Since then I had been observing the development of Goebel's S-matrix approach to the strong coupling theory from up close. As I have mentioned before, I even contributed to it by formulating and extending Goebel's theory in the form of an operator algebra including multi-partial waves. Through this work, I became acquainted with Gregor Wentzel and I was even introduced as his grandson at his retirement dinner party in Chicago, since Goebel was his student. But, to tell the truth I had never seriously studied his field theory of the strong coupling model. When I learned the Faddeev-Popov trick, it occurred to me to develop a functional-integral formulation of the strong coupling theory by using this trick. Because of other work that had to be done meanwhile, I could not even get started on this project until I had found two students, Gustavo C. Branco and Pavao Senjanovic, at the City College. At the beginning I thought that the problem was rather easy and one that was appropriate for graduate students. It turned out, however, that we had to overcome many obstacles; of which some were crucial albeit most were technical. I remember that I had to read Tomonaga's strong coupling paper again very carefully. At the end we succeeded in the functional-integral formulation of the strong coupling theory. There were two important general issues involved in this work, namely, (a) the introduction of collective coordinates in field theory, and (b) the semi-classical expansion in field theory. But, I suspect that at that time we did not fully recognize the relationship between the collective coordinates and the zero modes, nor that between the strong coupling limit of the static models and the classical limit.

In the summer of 1974, I went to Europe to attend the International Conference on High Energy Physics in London. Before the conference I stayed in Orsay for a month. During this time, influenced by a seminar given by Neveu on his work with Dashen and Hasslacher, Gervais and I decided to work on the semi-classical quantization of classical solutions, in particular the Nielsen-Olesen vortex solution of the Higgs model. Gervais studied our strong coupling paper very carefully and brought his new insights to bear upon it. In this summer we worked together in Aspen and at Brookhaven successively for several weeks to finish up this work on the quantized relativistic string as a strong coupling limit of the Higgs model. In this and in the subsequent work on soliton quantization, we used the Faddeev-Popov trick to extract the collective coordinates out of bosonic field theories. With this as the starting point, Gervais and I, together with Antal Jevicki (then a student) had firmly established the collective coordinate method as a method of semi-classical expansion in field theories, by the time of the following year's workshop on "Extended System in Field Theory" held at the École Normale Supérieure.

In these works we performed point canonical transformations in the functional-integral representation of bosonic quantum field theories. Since in functional-integral representations the operator ordering is not explicit, one often misses a term which is proportional to \hbar . Sure enough, we missed such a term in our work as was pointed out by E. Tombolis. Although Gervais and Jevicki have shown subsequently that it is possible to incorporate operator ordering into the functional-integral formalism, I realized that this was a serious drawback in the actual application of the functional-integral formalism. It was then that I decided to use the Hamiltonian operator formalism whenever a change of variables in quantum mechanics was involved.

Meanwhile, I had received a paper from Kikkawa, who had returned to Japan the previous year; the Hosoya-Kikkawa paper on the gauge theory of collective coordinates. The main idea of this paper was to construct for a given theory an artificial gauge theory, which involved the collective coordinates as gauge parameters, such that if one fixed the gauge by setting the collective coordinates zero, the theory reverted back to the original theory. A natural question occurred to us: what would happen if one applied this method to a genuine gauge field theory? It turned out that the most of the collective coordinates were absorbed into the vector potentials by gauge transformations except for the collective coordinates at the boundary of the system, which manifested themselves as surface variables. Subsequently we, Gervais-Sakita-Wadia, found that these surface variables were indispensable for a gauge invariant quantum mechanical description of the 't Hooft-Polyakov-Julia-Zee monopole-dyon solution. I encouraged Spenta Wadia (then a student) to investigate this problem further, addressing non-Abelian gauge theories in general, by using the Hamiltonian formalism.

At about this time Gervais and I worked together fairly regularly. According to my notes, we worked together in New York, Aspen, and Paris for a total of twelve weeks in two years ('76-'77). We developed the many-variable WKB method, the $A_0 = 0$ canonical formalism for non-Abelian gauge theories, and together with H. J. de Vega, a real time approach to instanton phenomena. In our group at the City College around this time, R. N. Mohapatra, who succeeded Kikkawa, was actively working with his students on his left-right symmetric model of weak interactions. Michio Kaku was productive in conformal supergravity with Townsend and van Nieuwenhuizen of Stony Brook. The main theme of the research surrounding me was the non-perturbative study of non-Abelian gauge field theories. Tamiaki Yoneya (then a postdoc) and Spenta Wadia were very active. I recall one of their works that dealt with the role of surface variables in the vacuum structure of Yang-Mills theory. In this paper, they explicitly transformed the Belavin-Polyakov-Schwarz-Tyupkin solution to the Coulomb gauge. They obtained a pendulum equation for the transformation function, which led to infinitely many solutions. This infinite multiplicity is now known as the Gribov phenomenon, but their paper predates that of Gribov by almost a year.

Large N QCD had been introduced by G. 't Hooft in 1974. One of his motivations was to remedy the arbitrariness involved in the "fishnet" diagrammatic approach to the string theory of strong interactions. This and the subsequent developments influenced us into thinking about the large N expansion in field theory in general.

In the winter of 1978-79, Wadia, then a postoc at the University of Chicago, came back to New York and informed me of a work with Eguchi. This discussion stimulated me to think about a gauge invariant calculation of non-Abelian gauge theories. I tried to rewrite a non-Abelian gauge theory in terms of equal-time Wilson loop variables. For this purpose I used the Hamiltonian canonical formalism and the method of change of variables that I had learnt long ago in Nagoya. To my surprise I obtained a field theory of interacting strings as a large N limit of the $SU(N)$ gauge theory. In order to justify the procedure, I applied the same procedure to known examples: a collection of many

identical free harmonic oscillators, and high density bosonic plasma oscillations. It worked correctly only if I made a similarity transformation such that the resulting Hamiltonian became hermitian. I was mystified by this until I spoke with Jevicki, then a postdoc at the IAS, who pointed out at once that this transformation essentially took care of the contribution from the Jacobian of the change of variables. So, I proposed to him that we write a joint paper on the general theory of the collective field method, so named later, after polishing up all the calculations. When I was writing a first draft of the paper in the summer of 1979, Jevicki informed me of the work of Brézin-Itzykson-Parisi-Zuber on the large N quantum matrix model, to which we could apply our method. Indeed, it was not difficult to derive their result by the collective field method.

As I have mentioned above, in Nagoya I had learned the method of change of variables used in the collective field theory. I was, and still am, curious as to whether I had a prototype of the collective field theory in my master's thesis or not. Sometime later when I went back to Nagoya, I went to the library to look for the thesis. However it was missing from the library.

In the fall of 1983 Gervais stayed in the City College for a few months as a visiting Professor to fill in the gap created by the departure of R. N. Mohapatra. I do not remember why, but I was explaining to him the derivation of the strong coupling group and its representation by the method of group contraction, namely the old work of Cook-Goebel-Sakita. To my surprise, he was visibly excited. He said “ This could just be the large N QCD.” It did not take us long to realize the relation between the large N baryons and the strong coupling theory: $\sqrt{N} \approx G$, once we learnt that Witten had shown that in the large N QCD the masses of baryons are of order N while the meson-baryon Yukawa couplings are of order \sqrt{N} . We spent several more weeks to complete the work, as we tried to establish its relation to the solitonic Skyrmin physics: $\sqrt{N} \approx G \approx 1/g$. I remember, when we had finished the work, that I was extremely satisfied with it, since it involved many of my previous works; $SU(6)$, strong coupling algebra, and soliton quantization, which were seemingly unrelated until then.

In the fall of 1980 I spent 4 months at the Yukawa Institute in Kyoto. On the way to Japan I was in Europe for several weeks and I came across the Parisi-Wu stochastic quantization paper in CERN's preprint library. Although I had been interested in statistical mechanics, I had never seriously worked on the subject. When I came back to New York the next spring I decided to spend some time in studying non-equilibrium statistical mechanics and stochastic processes in particular. My source was a Japanese book by R. Kubo and M. Toda entitled *Statistical Physics*, one of the Iwanami series, which I had bought in Japan. I translated one relevant chapter on stochastic processes and distributed it among my students, Guha, Alfaro and Gozzi, as they were all fascinated by stochastic quantization. Over the next few years several papers on stochastic quantization appeared from our group: on large N reduction by Jorge Alfaro and me, on supersymmetry and stochastic quantization by Ennio Gozzi, on stochastic quantization of supersymmetric theories by Kenzo Ishikawa, and the calculation of the chiral anomaly by Rodanthi Tzani.

At about this time I taught on two occasions a special topics course on Field Theory

and Statistical Mechanics, which included such topics as the derivation of the Landau-Ginzburg equation in the BCS model and the Lee-Low-Pines theory of polarons in terms of Feynman's variational method. Based on the lecture notes compiled by the students, I wrote a book called *Quantum Theory of Many-Variable Systems and Fields*, World Scientific Lecture Notes in Physics Vol.1, published in 1985.

In the spring of 1985 I met Zhao-bin Su of the Institute of Theoretical Physics in Beijing, who was a visitor to our condensed matter theory group at City College. He introduced me to several topics in condensed matter physics and frankly revealed the problems he was facing. One of them concerned the charge density wave transport phenomenon in a one dimensional system of electrons in a crystal. During the exchange of questions and answers, we gradually realized the importance of chiral symmetry and the chiral anomaly for this phenomenon. This work, together with a later work with Kenichi Shizuya on the same subject, drastically changed the level of my understanding of the chiral anomaly and the physics of anomalies in general. Another topic Su had introduced me to was the fractional quantum Hall effect. I have spent a fair amount of time and energy on this subject over the past ten years, and have written several papers with him, but I must confess that to this date I still do not understand the subject to my satisfaction.

In the past ten years I have become more and more interested in many-body problems, which is the subject I was involved in when I was a student at Nagoya. Interestingly, to me, the subject is rich enough to fill the gap between condensed matter physics research and particle physics research. When I learnt about the W_∞ algebra in a seminar on string theory, I realized that I had obtained the same algebra in my study of the fractional quantum Hall effect. I simply had not thought about the significance of the algebra in the physics of the Hall effect. In this respect I am pleased with a series of works I have done over the past five years with Dimitra Karabali, Satoshi Iso, and Rashmi Ray, since all of these works illuminate the significance of the W_∞ (or w_∞) algebra in the physics of low dimensional fermionic systems.

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